

erick L. Lewton, September 4, 1894, and preserved in the herbarium of the New York Botanical Garden is to be referred to *Asplenium auritum* Sw., a rather variable West Indian and Middle American species, not reported hitherto from the United States. The sheet in question is not of the typical Jamaican form, but is identical with Porto Rican material represented by Sintenis, No. 4616, which does not, however, seem specifically distinct.

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REVIEWS

Evolution and Adaptation

The reader in search of evidence corroborative of the origin of species by natural selection, or by direct adaptation will find but little comfort in Professor Morgan's recent book on evolution and adaptation.*

This author concludes that "Animals and plants are not changed in this or that part in order to become better adjusted to a given environment, as the Darwinian theory postulates." He holds that natural selection is not the moulding force that directs the development, or the origin of new forms, since among other proofs he points out that many have organs that are much less perfect than necessary, or more perfect than required by existence in a given environment.

Although new species are supposed to arise by the cumulation of minute fluctuating variations, according to the theory of natural selection, yet it is recalled that artificial selection, taking advantage of such variations, has never resulted in the formation of a new species although this method has been skilfully and rigidly applied for long periods of time. It is likewise pointed out that actual indisputable proof that any acquired character is capable of being inherited has not yet been brought forward. This is a phase of the subject comparatively easy of proof if true, and Professor Morgan is certainly justified in holding that mere asser-

* Morgan, T. H. Evolution and Adaptation. Svo. pp. xiii + 470. f. 1-7. 1903. The Macmillan Company, New York.

tions and arguments are futile in dealing with a problem the solution of which is directly accessible to the experimentalist.

The author's general position could hardly be better expressed than by the following, final paragraph of his book. "If we suppose that new mutations and 'definitely' inherited variations suddenly appear, some of which will find an environment to which they are more or less well fitted, we can see how evolution may have gone on without assuming new species to have been formed through a process of competition. Nature's supreme test is survival. She makes new forms to bring them to this test through mutation, and does not remodel old forms through a process of individual selection."

The essential feature of the book, and the one that constitutes its chief claim to attention consists in the fact that the author has brought the accumulated data of his extended researches upon growth and regeneration to the test, and finds that the theories of natural selection, inheritance of acquired characters, and origin of new forms by direct adaptation are inadequate in their interpretation, while the results in question are entirely in accord with evolutionary procedure by mutation, or discontinuous variation. The power to replace lost organs, or rebuild tissues that have been destroyed, the possession of useless or injurious organs, and the incipient stages of a new organ, or the atrophied form of an old organ may be accounted for by mutation, while the interpretation of these features has been one of the ever present difficulties in maintaining the theory of natural selection. The author, as may be seen from the above, is therefore in practical accord with de Vries, the results of whose recent experiments he discusses in detail.

Professor Morgan has certainly invited grave adverse criticism on certain features of his book. The subject involves a rigid analysis of phenomena of both animal and plant life, yet we do not find that any botanist passed upon the validity of the botanical statements, so far as the acknowledgments in the preface may be relied upon. A perusal of the volume reveals ample confirmation of this fault in the preparation of the book. To say that the color of a flower is a device that secures the visits of certain in-

sects is to rehearse a timeworn, popular, but non-scientific conclusion. Again it has been established beyond all doubt that etiolative elongations of plants in darkness are not adaptations, and are in fact exhibited by a scant and meaningless majority of species. The exaggerated thickenings and elongations of etiolated organs are due simply to morphogenetic disturbances, the utility of which is in some cases pure accident. The diurnal movements of leaves are recognized as useful by the author, but he ignores the well-known facts as to the benefit of nocturnal movements of the same organs. After the same manner, botanical equations set forth by Darwin, long outlawed by the progress of the science are rehearsed and annihilated to demonstrate the weakness of natural selection. A few hours' consultation with a working botanist would have eliminated these crudities from a book, which for the most part deals clearly and sanely with the questions taken under consideration.

D. T. MACDOUGAL.

CORRESPONDENCE

Linnaeus' Work on Ferns

EDITOR OF TORREYA :

There is an article in the October number of this journal in which an account of Linnaeus' work on ferns and his herbarium has been given, an account which contains, as it seems to me, several erroneous statements, which I cannot abstain from correcting.

I shall not enter upon any discussion about whether Linnaeus were the originator of binominal nomenclature, for this question has been settled long ago by a number of able writers in the "history of Botany"; nor shall I make any attempt to defend "the miscalled Father of Botany" (p. 147), "who must ever plead guilty to the charge of needlessly changing names already given by his predecessors" (p. 150)!

But what I wish to take up is the manner in which the author of the article, cited above, has interpreted Linnaeus' method of